

Interactive comment on “Remote sensed and in situ constraints on processes affecting tropical tropospheric ozone” by B. Sauvage et al.

Anonymous Referee #1

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General comments.

This paper is a well-written, high-quality modeling/data analysis of processes affecting tropical tropospheric ozone. One of the strong points of this work is the simultaneous assessment of multiple satellite and in-situ measurements of ozone and its precursors in a modeling framework well suited to assess the sensitivity of ozone profiles to various processes and uncertainties in emissions. The characterization of the uncertainties in various parameters and ranges in calculated values are quite helpful for allowing the reader to assess the precision of the various calculations. The relatively consistent improvement in magnitude and variation of ozone compared to a variety of measurements in most, but not all cases, provides significant evidence that the model processes are correct within the stated uncertainties. The bounding limits on the light-

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ning NO_x influence in the context of much larger scientific uncertainty of that process is welcome, particularly in the context of the authors' meteorological analyses and the scaling to the LIS/OTD measurements. Likewise the sensitivity to heterogeneous reactions involving HNO₃ and HO₂, while not as convincing as the other sensitivity studies, remains a contribution to the literature. With the model/measurement agreement approaching the measurement uncertainties in most cases (MAM and SON CO at Dubai and SON ozone at Ascension are curious exceptions) the fidelity of these calculations is remarkable.

This paper is suitable for publication in ACP following revision to address the following comments:

- 1) Abstract: "biomass burning inventory is larger by a factor of 2". Larger than what?
- 2) Pg.2, 1st para: replace 'confounded' with 'limited'.
- 3) Pg. 3, 2nd para: How does GOME provide NO₂ in cloudy pixels? The effect of using pixels with 50% or 70% (which is it?) cloud fraction could be significant to the reported tropospheric NO₂ amounts.
- 4) Pg. 4, Sect. 3.2: The terminology "Standard (improved)" is confusing. Original and improved would make sense; however, anticipating third and subsequent versions, the authors might prefer to give the model configurations version numbers.
- 5) Pg. 7, 3rd para: The disagreement in SON ozone at Ascension (figs 5 and 10) begs for an explanation.
- 6) Pg. 8, sect 4.1.2b: the suggestion of a seasonal variation in the IC/CG ratio should be clearly identified in the conclusions as a point of interest for further study.
- 7) Pg. 8, sect. 4.2.1: The HCHO (and NO₂ elsewhere) model/GOME correlations are remarkably high. The authors should clearly describe the relationship between the GOME retrievals and any a priori constraints that might influence the model/measurement correlations.

8) Pg. 9, end of 4.2.2: The authors should consider elucidating the point that the Scan-angle Method is the only one of several satellite tropospheric techniques that captures the correct seasonal variation of tropical tropospheric ozone over Africa. The suggestion that the vertical profile of ozone and instrument sensitivity to that vertical distribution may likely be the key aspects of capturing the true variation. Perhaps there really is no paradox.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11465, 2006.

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